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E. J. STONE, M.A., F.R.S., President, in the Chair.

Charles Henry Clarke, B.A., The International College, Spring Grove, Isleworth;
Joseph Morrison, M.A., M.D., Office of the *American Ephemeris*, Washington, U.S.A.; and
Major C. Hawkins Fisher, The Castle, Stroud, Gloucestershire;

were balloted for and duly elected Fellows of the Society.

On the supposed possible Independence of Bessel's and Le Verrier's Expressions for the Mean Longitude of the Sun subject to the Conditions imposed upon them by their Use in practical Astronomy in the Determination of Mean Solar Time. By E. J. Stone, M.A., F.R.S.

The real point at issue between me and those who differ from my views with respect to the principal cause of the large existing discordances between Hansen's Tables of the Moon and observations, is as follows. Can we, consistently with the uses we have made of Bessel's and Le Verrier's expressions for the longitudes of the mean Sun, consider that there are two independent mean Suns without any necessary physical connection? That the question really resolves itself into this is easily shown. For let S denote the length of the sidereal day, and

$$T_1 = (1 + x_1)S, \quad T_2 = (1 + x_2)S,$$

whatever values be assigned to x_1 or x_2. Then if ω_1, n_1, ω_2, n_2, are the angular velocities of the Earth about its axis, and the *true* Sun's mean motion in longitude in the units T_1 and T_2 respectively, we have identically

$$\frac{\omega_1}{\omega_2}=\frac{n_1}{n_2}=\frac{T_1}{T_2}.$$

This follows from the very definition of angular velocities and units of time, and therefore

$$\frac{T_1-T_2}{T_2}=\frac{n_1-n_2}{n_2}$$

which is my result; and one which must be true if n_1 and n_2 refer to any continuous motion.

But if we determine x_1 and x_2 so that

$$2\pi x_1=\alpha_1, \quad 2\pi x_2=\alpha_2,$$

α_1, α_2 being the circular measures of two angles; then we have, since 2π is the value of ω when S is the unit,

$$\omega_1=2\pi(1+x_1)=2\pi+\alpha_1$$
$$\omega_2=2\pi(1+x_2)=2\pi+\alpha_2.$$

$$\therefore \frac{\omega_1}{\omega_2}=\frac{T_1}{T_2}=\frac{2\pi+\alpha_1}{2\pi+\alpha_2}$$

$$\therefore \frac{T_1-T_2}{T_2}=\frac{\alpha_1-\alpha_2}{2\pi+\alpha_2}=\frac{\alpha_1-\alpha_2}{\omega_2}$$

$$\therefore \frac{T_1-T_2}{T_2}=\frac{\alpha_1-\alpha_2}{\omega_2}=\frac{n_1-n_2}{n_2}.$$

My result is therefore not inconsistent with, but identical with, that which has been previously obtained, for of course we can, if we please, assume

$$L_1=A_1+\alpha_1 t$$
$$L_2=A_2+\alpha_2 t,$$

and call L_1 and L_2 the longitudes of the mean Suns (1) and (2).

The real divergence between my views and those which have hitherto been accepted arises at this point; can we found our measures of mean solar time upon the expressions

$$L_1=A_1+\alpha_1 t, \text{ and } L_2=A_2+\alpha_2 t,$$

and change from the one to the other, when they are used, as we do use them, without breaks in the continuity of our measures of time? or without the changes in the units which my theory indicates?

If this interchange of expressions be possible without any

breaks of continuity in our measures of time or without equivalent changes in all our *adopted mean motions of the Moon and planets*, we can assume without error

$$\alpha_1 = \text{Bessel's adopted value},$$

$$\alpha_2 = \text{Le Verrier's adopted value},$$

and still infer that

$$\frac{T_1 - T_2}{T_2} = \frac{\alpha_1 - \alpha_2}{\omega_2} = \frac{n_B - n_{LV}}{\omega_2};$$

but if the two mean Suns cannot, consistently with the use which we have made of them, be regarded as independent, then it is quite impossible to have both $\alpha_1 = n_B$ and $\alpha_2 = n_{LV}$, as the discordance between my result and that usually accepted shows immediately.

Now, a conditional relationship between the two Suns is at once established when a *third* real continuous motion (that of the Moon, for instance) is connected with the motions of the centre of gravity of the Earth and that of the rotation of the Earth about its centre of gravity and expressed in terms of t. So long as we are dealing with the *two* independent motions, that of the centre of gravity of the Earth and that about the centre of gravity of the Earth, there is no absolute necessity of separating the concrete factors ω and t, and n and t, in the discussion of the ratio of the angular velocity of the Earth to that of the Sun's mean motion in longitude. But this separation has to be made before we can discuss the Moon's motion, or, indeed, our own motion with reference to its position in space. This is the point which, I believe, has been overlooked by mathematicians. It is one which might certainly not present itself naturally to a mind occupied only with the Solar Theory, and it presented itself to my mind simply because I was examining the Solar Theory with direct reference to the bearings of questions of time upon the Lunar Theory. Mathematically expressed, what I have been stating amounts to this.

If the Sun's mean longitude

$$= \epsilon + nt, \qquad (1)$$

and we find from our investigations of the Lunar Perturbations that the Moon's longitude

$$= A + Nt + \text{Periodic terms}, \qquad (2)$$

then N and n are necessarily referred to the *same* unit of time, *whatever that unit may be*; and the angle n, assigned or supposed to be known, is such that in *each unit of time* the Sun's mean longitude increases n. But before our Lunar Theory can be compared with the observations, we require the theoretical expressions for the observer's position as well as those for the centre of gravity of the Moon. As a matter of fact we do not, in our theoretical investigations, introduce any such thing as a

mean Sun as distinguished from the true Sun, and we employ the same numerical value n, which expresses the Sun's mean motion in longitude, to establish the relationship between the motion of the centre of gravity of the Earth and the motion about that centre of gravity. The distinction, therefore, between the longitude of any mean Sun and the mean longitude of the true Sun is one not recognised in our theoretical work; and as we use, not an imaginary Sun, but the real Sun for establishing the relationship between the motion of the centre of gravity of the Earth and the motion about the centre of gravity of the Earth, it follows that if we do determine our mean time by equating the motion of the meridian to a mean Sun, then that mean Sun must be the true Sun in order that the times thus determined may be those upon which our theories are constructed, and for which they alone can be true.

It has been usual to eliminate the angular velocity of the Earth about its axis, from our equations, by supposing

$$\omega = 360 \times 60 \times 60'' + n. \qquad (3)$$

But the equations (1), (2), and (3) cannot co-exist except for *one* definite and determinate angle n; but if T be the unit of time in which the Sun's mean longitude does increase n, and the angle through which the Earth has turned about its axis increases $360 \times 60 \times 60'' + n$, we can at once find the true corresponding equations when we adopt any other angles n_1 or n_2 instead of n for the Sun's mean motion in longitude. For if T_1 and T_2 are two times expressed in terms of T such that $nT_1 = n_1$ and $nT_2 = n_2$: then if we adopt T_1 and T_2 as our units of time the angular velocities of the Sun in mean longitude will be n_1 and n_2 respectively; and conversely, if we adopt n_1 and n_2 for the Sun's mean motion in longitude, then the units of time must, consistently with the truth of (1) and (2), be T_1 and T_2.

If, therefore, t_1 and t_2 denote the times, when n_1 and n_2 are adopted, which correspond to the same *absolute instant* as that indicated by t when n is adopted, we have

$$\text{Sun's mean longitude} = \epsilon + nt = \epsilon + n_1 t_1 = \epsilon + n_2 t_2$$

$$\text{Moon's mean longitude} = A + Nt = A + N\frac{n_1}{n} \cdot t_1 = A + N\frac{n_2}{n} \cdot t_2$$

and

$$\text{R.A. of meridian in arc} = \epsilon + \omega t = \epsilon + (360 \times 60 \times 60'' + n)t$$

$$= \epsilon + (360 \times 60 \times 60'' + n_1)t_1 + 360 \times 60 \times 60''\left(\frac{n_1 - n}{n}\right) \cdot t_1$$

$$= \epsilon + (360 \times 60 \times 60'' + n_2)t_2 + 360 \times 60 \times 60''\left(\frac{n_2 - n}{n}\right) \cdot t_2$$

Converting these expressions into time in the usual way we have

$$\text{R.A. of meridian} = C + \left(24^h + \frac{n}{15}\right).t$$

$$= C + \left(24^h + \frac{n_1}{15}\right).t_1 + 24^h\left(\frac{n_1 - n}{n}\right).t_1$$

$$= C + \left(24^h + \frac{n_2}{15}\right).t_2 + 24^h\left(\frac{n_2 - n}{n}\right).t_2$$

Now, if n_1 and n_2 are the two values of the Sun's mean motion assumed by Bessel and Le Verrier, and B and V denote their adopted values of the sidereal times at corresponding mean noons, when $24^h.t_1 = 24^h \times$ some integer, and $24^h.t_2 = 24^h \times$ same integer, we shall have

$$\text{True R.A. of meridian at Bessel's mean noons} = B + 24^h\frac{n_1 - n}{n}.t_1$$

$$\text{,, ,, Le Verrier's mean noons} = V + 24^h\frac{n_2 - n}{n}.t_2$$

The difference, therefore, which arises from the use of Le Verrier's expression instead of Bessel's is

$$= V - B + 24^h.\frac{n_2 - n_1}{n}.t.$$

The term

$$24^h.\frac{n_2 - n_1}{n}.t$$

has been usually neglected on the impossible assumption that we could use either Bessel's or Le Verrier's expressions for the sidereal times at mean noon, without allowing for the change in the direction of the meridian between the two noons; and it has been the principal, if not the only argument against the acceptance of my views, that when you do neglect the terms

$$24^h.\frac{n_1 - n}{n}.t_1 \quad \text{and} \quad 24^h.\frac{n_2 - n}{n}.t_2,$$

which I have proved to be necessary corrections to B and V before the true mean times can be correctly deduced from the sidereal times, the times thus incorrectly found only differ by the 366th part of the difference indicated by my theory. If we suppose Le Verrier's value n_2 to be the true value, a supposition which will give the true difference, we have when the times were determined by B the true time

$$t_1 + \delta t_1 = t_1 + \frac{n_2 - n_1}{n_2}.t_1,$$

and if we determine, therefore, the Moon's mean motion with these defective times t_1 we shall find instead of N, the value $N + \delta N$ such that

$$N(t_1 + \frac{n_2 - n_1}{n_2}.t_1) = (N + \delta N)t_1$$

or

$$\delta N = \frac{n_2 - n_1}{n_2}.N,$$

and consequently, in 1864, when we replaced B by V, we ought to have changed the adopted value of the Moon's motion to the extent indicated; and all our other adopted mean motions should have been similarly changed to allow for the change in the adopted unit of time.

It is difficult to understand how any practical astronomer can seriously maintain that we are in these questions of mean solar time only concerned with the Sun's hour-angles, and not with the direction of the meridian in space. As a matter of fact our clock-errors are deduced from sidereal times which directly depend upon the position of the meridian with respect to the equinox.

The whole of our difficulties have arisen from attempts to carry on simultaneously an approximation to the Sun's mean motion in the true mean solar day, and to use that true mean solar day as our unit of time.

The equation $\omega T - nT = 2\pi$ has been theoretically treated as if the factor T, the adopted unit of time, did not exist, and as if the true equation was $\omega - n = 2\pi$. It has therefore been thought that the only possible way of keeping $\delta(\omega T) - \delta(nT) = 0$ was to take $\delta\omega = \delta n$, a condition which keeps the Sun's hour-angle unchanged, but throws the direction of the meridian of the observer out of its true position; such a solution cannot therefore be the true solution of our physical problem. Directly the unit T is introduced the difficulty disappears. All we require to keep the hour-angle unchanged is that

$$\delta\omega T + \omega\delta T = \delta n T + n\delta T,$$

which is satisfied if

$$-\frac{\delta T}{T} = \frac{\delta\omega}{\omega} = \frac{\delta n}{n},$$

and as these relations satisfy the necessary conditions that the Sun's mean longitude, and the direction of the meridian in space, should both be unaltered by the changes introduced into the adopted value of n, the solution thus obtained is the only possible solution which will bear comparison with the facts of observation.

Although it may appear useless to continue researches of which the fundamental principle is still disputed by astronomers of note, I may mention that, besides accounting for the missing $4''$ required to reconcile our observations with the theoretical value of the secular acceleration of the Moon's mean motion in longitude, I have convinced myself that the long inequality with a coefficient of $21''$ introduced by Hansen, but whose coefficient was found to be insensible by Delaunay, is, to very nearly its full extent at all events, an empirical inequality rendered necessary to reconcile theory and observation on account of breaks in the continuity of our measures of time similar to that which took place in 1864. I have not seen at present any evidence which weakens in the slightest degree my confidence in the accuracy of my results.

On the supposed Unreliability of the Greenwich Observations of the Sun in Mean Results. By E. J. Stone, M.A., F.R.S.

In the *Monthly Notices* for November there appears a paper by my friend Mr. Dunkin on the "Greenwich Mean Tabular Errors of the R.A. of the Sun." The paper is an interesting one, as showing examples of considerable personalities between the different Greenwich observers. But the conclusion to which Mr. Dunkin is led that the apparent increase in the Tabular Errors in R.A. of the Sun since 1864 is due solely to these personalities, is one which appears to me unwarranted by the facts. The subject which Mr. Dunkin has attempted to treat is one of very great difficulty indeed.

With respect to personal errors which can be quantitatively determined directly, I am strongly of opinion that corrections should be applied to reduce each observer's work to the mean habit of all the regular observers; but with respect to personalities which can only be determined indirectly by assuming that the errors of the Tables disappear in the mean of a considerable number of results, I seriously doubt whether any great advantages are likely to result from their application.

But whatever may be the value of my opinion on the point, the usage at Greenwich has been to trust to mean results without any attempts, so far as I am aware, to correct for personalities except in the determination of clock-rate. The effect of this is to impress the mean habit of the general body of the observers upon the fundamental data; and consequently, although the Greenwich results are, in my opinion, thoroughly reliable and of great accuracy as mean results, the individual observations of a selected observer, instead of being free from personalities, as Mr. Dunkin seems to suppose, are generally more affected by such personalities than the general mean. This point has frequently been overlooked, but I believe that there is little doubt of its importance in the present case. The observation of the R.A. of the Sun consists of a difference between the times of transit of the centre, as determined from the times of transit of the limbs, and some one star corrected for clock-rate applied to some adopted Right Ascension of the star. In practice there will, of course, be usually several stars observed and the mean result taken. If, therefore, an observer has any distinct personality in such observations, this will prevent his so-called observed R.A. from agreeing with the tabular error given by the other observers, but the exact error which he gives will depend also upon the adopted Right Ascensions, and we can make it positive or negative, large or small, by adopting different values of the fundamental clock-stars.

Independently, therefore, of personal changes, there are to be considered the changes in the adopted clock-stars. Now, it has been the plan at Greenwich to discuss *all* the solar observations

of each year, for the determination of the corrections required by the adopted clock-stars, in order to refer them to the true mean equinox of the year. Such corrections to epoch are necessarily affected by the general mean habit of the body of the observers employed on the observations. From time to time the stellar observations of several years are collected and reduced to some common epoch, and corrected for the mean of the corrections deduced for the several years from the solar observations. These Sun observations have therefore become fundamentally incorporated in the Greenwich work; and if untrustworthy in mean results, the consequences are serious indeed. Now, the observations 1864 to 1869 were all based fundamentally upon the Right Ascensions of the 1860 Catalogue, which embraces the observations made from 1854 to 1860, and is affected by the mean correction to epoch of the observers who made these observations.

The observations 1870–1877 are based fundamentally upon the Right Ascensions of the 1864 Catalogue, and are affected by the mean correction to epoch of the observers during the period 1860–1867; whilst the observations made 1878–1881 are based on the standard places of the Nine-Year Catalogue, which embraces the observations made 1868–1876, and are affected by the mean habit of the observers during that period. It is clear, therefore, that if there have been such serious changes as those which Mr. Dunkin supposes to have taken place in the mean habit of the general body of the observers, then Mr. Criswick's isolated observations should have shown changes in the tabular errors of the Sun during the period 1864–1881, unless his personal habit has also changed. On the other hand, if there have been no such great changes in the mean habit of all the observers, then the mean errors in longitude are trustworthy results. The only real test of any considerable change in the mean habits of the observers is to see whether there is, or is not, any great change in the correction to epoch when stars are brought up from one epoch to another with precession constants and proper motions which are supposed to be known with sufficient accuracy not to seriously affect the observed differences. If we apply this test to the observations 1864–1869 and 1878–1881, we find the mean correction to epoch for the first period is $-0^s{\cdot}020$; whilst that for the second period is $-0^s{\cdot}015$. The difference is only $0^s{\cdot}005$ or $0''{\cdot}075$; and it is clear that there is, so far as we can thus check the point, no indication of any serious change of mean habit, such as Mr. Dunkin assumes to have taken place.

The principle of separating the different observers' work without referring to the mean equinox of each observer is, I feel certain, radically unsound. So far as materials exist for the inquiry, it appears clear to me that Mr. Criswick's observations do show an increase of tabular error between 1866·5 and 1879·5 when reduced to his own epochs. It is probable, but not quite certain, that the observations 1878–1881 when referred to their own equinox should be increased by about $0''{\cdot}24$ relatively to

those about 1864, on account of the addition now made to the reading for the line of collimation at Greenwich for effects of observations through the cubes.

So many changes were made during the period 1870 to 1877, and these changes have become so mixed up in the fundamental determinations, that it appears almost hopeless to attempt to disentangle their effects; but probably in mean results the errors are exceedingly small. It appears clear to me that unless we can trust the general mean results the Greenwich observations can afford us no reliable information on the question raised, and it is certainly illogical to assume that these Sun observations are sufficiently accurate to determine the corrections to epoch, and then afterwards to maintain that the mean results are unreliable.

The mean errors of the tables in longitude for the separate years, and the mean errors for the groups of years, 1864–1869, 1870–1877, 1878–1881, which result from the observations made by all the observers, are as follows:—

Year.	Mean Error.	No. of Obs.	Mean Error of Group.
	"		
1864	+0·025	122	
1865	+0·248	122	
1866	−0·109	109	
1867	−0·203	93	+0·080
1868	+0·245	131	
1869	+0·272	98	
1870	+0·122	126	
1871	+0·306	113	
1872	−0·037	120	
1873	+0·408	124	
1874	+0·762	106	+0·456
1875	+0·728	110	
1876	+0·675	112	
1877	+0·683	87	
1878	+0·816	85	
1879	+0·748	77	
1880	+0·861	115	+0·79
1881	+0·759	124	

On the Change in the adopted Unit of Time. By Major-General J. F. Tennant, R.E., F.R.S.

Mr. Stone's papers on the cause of the large error in our modern Tables, and the criticisms I have seen on them, have led me to consider the subject; and I put my idea on paper because it may simplify the question to some. The result is the same as Mr. Stone's, and the reasoning, though independently thought out, is in much the same line as part of his second paper in the *Monthly Notices.*

For many years past the use of the Sun for the purpose of regulating clocks in an Observatory has ceased. Practically all observations are made in Sidereal Time, and the Mean Time where required has been deduced from it; and even where series of observations have been used in the investigation for Tables the Times have been reinvestigated, the clock-errors having been determined by the observations of stars. Therefore the Mean Times used have essentially depended on the length of the mean solar day in seconds of sidereal time. The Julian year taken at 365·25 mean solar days therefore depends on this time. The relation between the solar and sidereal days has been practically till recently that determined by Bessel, and it is from observations dated in what we may call Besselian Mean Time that the investigations of physical astronomy have deduced the data of modern Tables.

One of the discoveries from these investigations has been that the mean motion of the Sun and the length of the mean solar day in sidereal seconds dependent on it were not sufficiently accurate. The result of introducing the new Tables has been that, while the sidereal day and second are unchanged, the unit of Observatory Mean Time has become the Leverrian solar day* and year instead of the Besselian, and there has also been a small jump. Now, the data of, say, Leverrier's Tables having been determined from observations dated in Besselian Time should be consistent with fresh observations so dated. When compared with those dated by the Observatory clocks they do not agree, there is an apparent error of the Tables, which is really due to the fact that we are comparing a theoretical state of things at a Besselian date with the actual state at a Leverrian date nominally the same.

The Epoch of Leverrier's Elements is Jan. 1·0 1800 in Besselian Time, which is not the same physical instant as Jan. 1·0 1800 of Leverrian Time or that of the present *Nautical Almanac*, and the Elements require correction for the difference.

* The Mean Year or Mean Interval between two passages of the Sun through the first point of *Aries* is determined by observation as 366 Sidereal days $+n$ seconds, and this necessarily equals 365 Mean Solar days $+n$ Solar seconds. If n be wrongly determined, the change will not be in the length of the Sidereal day and second, but in that of the Solar day and second.

The mean motions, again, are for Besselian mean days and Julian years, and to make them correct for the dates of our present *Nautical Almanac* they require to be multiplied by

$$\frac{\text{Sidereal Seconds in Leverrian Mean Day}}{\text{Sidereal Seconds in Besselian Mean Day}}.$$

Hansen's or other Tables of the Moon will require a corresponding set of corrections to adapt them to our Observatory Times.

It is, however, unnecessary to correct the Tables. All we need do is to recollect that the data in our present *Nautical Almanac* are the direct result of the Tables, and that they must be corrected for the difference between the Mean Time used in making the Tables and the Leverrian Mean Time of the Observatories. Thus, if the difference between the Mean Time which Hansen's Tables are referred to and Leverrian Time is n^s on any day, the latter being the smaller we must compare the Moon's Right Ascension at Transit with that which according to the almanac she would have had n^s after the Transit.

When the data of the Tables have been entirely determined from observations reduced as I have described, then the Tables require only this correction of date. But there have been used some isolated old observations such as eclipses of the Sun in the Lunar Tables and Transits of *Venus* and *Mercury* whose times can only be determined by solar observations, or are given by observers in Apparent Time without means of referring to stars. In these cases the Mean Times are Leverrian, or *True* Mean Times, within the limits of observation, and we should expect a discordance with the calculations made from the provisional data. By using them to correct the data of the Tables on the hypothesis that these times were Besselian, error has been introduced. A marked case is that of the Secular Acceleration of the Moon's Mean Motion which has been determined practically from very old eclipses compared with the Tables whose mean motion came from modern observations.

1883, *Oct.* 31.

Remarks on Mr. Stone's Explanation of the Large and Increasing Errors of Hansen's Lunar Tables by means of a supposed Change in the Unit of Mean Solar Time. By Prof. J. C. Adams, M.A., F.R.S.

In some recent communications to the Royal Astronomical Society Mr. Stone contends that the mean solar day in use before 1864—when Le Verrier's Solar Tables were substituted for Bessel's in calculating the sidereal time at mean noon given in the *Nautical Almanac*—differs from the mean solar day adopted since that time.

In the *Monthly Notices*, vol. xliii. p. 403, Mr. Stone states that the consequent error in our present reckoning in time is increasing at about the rate of $1^s.46$ per annum, and in the same volume, p. 335, he adduces this supposed error in explanation of the increasing errors of Hansen's Lunar Tables.

That this view of Mr. Stone's is erroneous may, I think, be shown by very simple considerations.

The only mean Sun known to astronomers is an imaginary body which moves uniformly in the equator at such a rate that the difference between its Right Ascension and that of the true Sun consists wholly of periodic quantities.

These periodic terms are due to the obliquity of the ecliptic, the eccentricity of the Earth's orbit, and also to the small perturbations of the Earth's motion about the Sun.

The difference between the Right Ascensions of the two bodies at any moment is called the Equation of Time.

The instant of Mean Noon is determined by the transit of this imaginary Mean Sun over the meridian of a given place just as the instant of Apparent Noon is determined by the transit of the true Sun over the same meridian.

Hence, the mean time, according to the definition of it above given, may be determined by observation of the transit of the true Sun over the meridian, subject only to the small error to which all transit observations are liable, and also to the extremely small error which is possible in the theoretical expression for the equation of time. When this mode of determining the mean time is employed, no accumulation of error in proportion to the interval of time from a given epoch is possible.

If, as it is frequently convenient to do, we wish to determine the mean solar time by means of the sidereal time supposed to be known, without having to make a transit observation of the Sun, we must employ the sidereal time at mean noon calculated from the proper formula or from the Solar Tables. This sidereal time at mean noon is equal to the Sun's mean longitude at mean noon corrected by the equation of the equinoxes in Right Ascension.

In order to find the mean time correctly in this way it is necessary to employ the correct value of the Sun's mean longitude, and any error in the assumed value of this quantity will produce an equivalent error in the mean time deduced.

Any such error can be at once checked and corrected by observation of the Sun's transit over the meridian.

If we wilfully refuse to check our results by solar observations, the error in the determination of the mean time by means of the sidereal time would, no doubt, increase in proportion to the interval of time from a certain epoch. Practically, however, it would be intolerable to use Solar Tables which were grossly erroneous, and long before the error of time became important the tables would be replaced by more accurate ones.

For many years previously to 1864 Bessel's formula had been

employed in the *Nautical Almanac* for the calculation of the sidereal time at Greenwich mean noon.

In 1864 the error of Bessel's formula amounted to rather more than half a second of time, and accordingly in that and subsequent years the sidereal time at mean noon was deduced from Le Verrier's Solar Tables, which gave much more accurate results.

Now it is contended by Mr. Stone that by the change thus introduced into the *Nautical Almanac* the unit of mean solar time was practically altered to such a degree that at the end of 1881 the difference in the count of mean solar time amounted to nearly 27 seconds, and that the difference is increasing at the rate of about 1·46 seconds per annum.

It is clear, therefore, that if no such change had been made in the *Nautical Almanac*—that is, if Bessel's formula had continued to be employed—no such change of the unit of time would have taken place.

Let us see then, what difference this would have made in the count of mean solar time as derived from sidereal time when compared with the count found by means of our present *Nautical Almanac*.

Bessel's formula for the sidereal time at Greenwich mean noon of Jan. 1 in any year is given in the prefaces to the *Nautical Almanacs* from 1834 to 1863 inclusive. In 1864 and subsequent years the sidereal time at Greenwich mean noon is derived from Le Verrier's tables.

The following little table shows the sidereal time at Greenwich mean noon of Jan. 1 as calculated for every fifth year from 1860 to 1885 by Bessel's formula, and as taken from the several *Nautical Almanacs*:—

	By Bessel's Formula.	From *Nautical Almanac*.		Diff.
	h m s	h m s		s
1860	18 41 28·87	18 41 28·87	Bessel's formulæ employed	0·00
1865	18 44 35·36	18 44 35·92	Le Verrier's Tables employed	0·56
1870	18 43 43·87	18 43 44·44	,, ,,	0·57
1875	18 42 54·47	18 42 55·06	,, ,,	0·59
1880	18 42 5·95	18 42 6·56	,, ,,	0·61
1885	18 45 11·73	18 45 12·37	,, ,,	0·64

Hence we see that the difference of sidereal times at mean noon in consequence of the change from Bessel's formula to Le Verrier's Tables, which amounted to $0^s{\cdot}56$ in 1865, had increased to $0^s{\cdot}64$ in 1885. That is, the difference increases at the rate of $0^s{\cdot}08$ in twenty years, or of $0^s{\cdot}02$ in five years.

But according to Mr. Stone's theory as shown in his tabular comparisons of mean solar times computed from sidereal times by means of the *Nautical Almanac* and of those sidereal times

"corrected to agree with Bessel's sidereal times," the differences would be as follows:—

	s		s
1865	2·0	1875	16·6
1870	9·3	1880	23·9

and at the end of 1881 the difference would have increased to $26^s{\cdot}8$; so that the increase in five years would be $7^s{\cdot}3$ instead of $0^s{\cdot}02$ as above. In fact the difference according to Mr. Stone's theory is just 365 times as great as it should be.

The origin of this enormous discrepancy between Mr. Stone's theory and the fact is readily seen by considering that mean solar time is measured, not by the Sun's mean motion in *longitude*, as Mr. Stone's theory supposes, but by the motion of the mean Sun in *hour angle*, which is about 365 times greater in amount. Hence any small error in the determination of the Sun's mean motion in longitude causes a proportionate error of only about a 365th part of the amount in the interval of mean solar time as inferred from the interval of sidereal time. In fact, if n denote the Sun's mean motion in longitude in a mean solar day, then the length of the mean solar day will be to the sidereal day in the ratio of

$$360° + n : 360°.$$

If now $n + dn$ denote another slightly different determination of the Sun's mean motion in longitude in a mean solar day, the ratio of the length of a mean solar to that of a sidereal day will become

$$360° + n + dn : 360°.$$

Hence the measure of a mean solar day when expressed in sidereal time will be increased in the ratio of

$$360° + n + dn : 360° + n,$$

or

$$1 + \frac{dn}{360° + n} : 1.$$

Since 360° is nearly 365 times n, this ratio will be

$$1 + \frac{1}{366}\,\frac{dn}{n} : 1 \text{ nearly},$$

Whereas, according to Mr. Stone's theory, this ratio should be

$$1 + \frac{dn}{n} : 1.$$

It has been already remarked that it is convenient practically to determine the mean solar time from the sidereal time, but in order to do this correctly, it is of course necessary

to employ the correct value of the Sun's mean longitude. At the present time Bessel's value of the Sun's mean longitude is about $0^s{\cdot}6$ in error, and therefore the mean solar time inferred by means of it from the sidereal time would be in error to the same amount. The mean longitude found from Le Verrier's Tables is much nearer to the truth, and therefore the mean solar time found from the sidereal time by using this value would be much more nearly correct.

It must not be forgotten however that, as we have already stated, the mean solar time may be derived from observations of the transit of the Sun over the meridian, without employing the sidereal time at all. Apparent solar time, which is found directly from observation of the Sun is converted into *mean* solar time by applying the equation of time, which is known from the solar theory, without reference to the sidereal time.

On the Change in the Unit of Time implied in the Substitution of Hansen's for Bessel's Expression for the Longitude of the mean Sun. By Prof. A. Cayley, M.A., F.R.S.

I have before me Mr. Stone's paper "An Explanation of the Principal Cause of the Large Errors &c.", *Monthly Notices*, vol. xliii. (1883) pp. 339–345, in which Mr. Stone arrives at the conclusion that the effect of the substitution of Hansen's expression for the longitude of the mean Sun in place of that of Bessel is to change the unit of time in the ratio of $1+x$ to 1, where

$$x = -\frac{0{\cdot}055871}{1296027{\cdot}074055}.$$

Prof. Adams considers, I think rightly, that the true value of x is about $\frac{1}{365}$ of this value.

I wish in the present paper to present the question in what appears to me to be the most distinct manner.

Bessel's expression for the longitude of the mean Sun, 1850 Jan. 1, Paris mean noon $+t$ is

$$l = 280^\circ\ 46'\ 36''{\cdot}12 + 1296027''{\cdot}618184\,.\,t + 0''{\cdot}0001221805\,.\,t^2,$$

say $\quad = \quad$ „ $\quad + Lt \quad$ „

the unit of time being *supposed* to be a Julian year of $365{\cdot}25$ mean solar days, and only the term in t being here attended to.

I infer that according to Bessel the Sun's mean hour-angle from the meridian of Paris would be

$$h, = 365{\cdot}25 \times 360^\circ\,.\,t,$$

viz. t increasing by $\frac{1}{365{\cdot}25}$ (=one mean solar day) h would increase by 360°. Say this value is $h = Ht$.

The Right Ascension of the mean Sun is equal to the mean longitude l; and hence the Right Ascension of the meridian of Paris is $h+l$,$=(\mathrm{H}+\mathrm{L})t$; or reckoning from a fixed equinox instead of the mean equinox, this is $h+l$—a term depending on the precession$=(\mathrm{H}+\mathrm{L}-\varpi)t$; $\varpi=50''{\cdot}21129(1-\cos\omega)$, where $50''{\cdot}21129$ is Bessel's value for the general precession.

Hence

$(\mathrm{H}+\mathrm{L}-\varpi)t=$ Bessel's value for Earth's rotation in t Julian years.

Similarly, Hansen's expression for the longitude of the mean Sun, 1850 Jan. 1 Paris mean noon$+t'$, is

$$l'=280°\ 46'\ 43''{\cdot}20+1296027''{\cdot}674055\,.\,t'+0''{\cdot}000\,f106850\,.\,t'^2$$

say $\quad=\quad$,, $\quad+\mathrm{L}'t'\quad$,,

the unit of time being *supposed* to be a Julian year of $365{\cdot}25$ mean solar days; and only the term in t' being here attended to.

I infer that according to Hansen the Sun's mean hour-angle from the meridian of Paris would be

$$h'=365{\cdot}25\times360°\,.\,t',$$

viz. t' increasing by $\frac{1}{365{\cdot}25}$ (=one mean solar day) h' should increase by 360°. Say we have $h'=\mathrm{H}'t'$. H′ is=H, but for convenience I use the accented letter.

As before, the Right Ascension of the meridian of Paris, reckoned from a fixed equinox, is $h'+l'$—a term depending on the precession,$=(\mathrm{H}'+\mathrm{L}'-\varpi')t'$ $\varpi'=50''{\cdot}224(1-\cos\omega)$, where $50''{\cdot}224$ is Hansen's value of the general precession.

Hence

$(\mathrm{H}'+\mathrm{L}'-\varpi')t'=$ Hansen's value of Earth's rotation in t' Julian years.

Now, Bessel's mean Sun is not Hansen's mean Sun, and a Julian year (Bessel) is not=a Julian year (Hansen). In order to determine the ratio of the two Julian years (or say the ratio of the two units of time) Mr. Stone compares the motions in longitude Lt, and L′t' of the two mean Suns; but this assumes that the two mean Suns are identical, *which, as just remarked, is not the case.* Admitting that they were so, and neglecting with Mr. Stone the difference of precession the ratio would be

$$\frac{\mathrm{L}'}{\mathrm{L}},\quad=1+\frac{\mathrm{L}'-\mathrm{L}}{\mathrm{L}},\quad=1+\frac{0''{\cdot}055871}{\mathrm{L}},$$

The proper equation for the comparison of the units is afforded by the expressions for the time of the Earth's rotation; consider a *definite* interval of time (=about one Julian year), and say this is the adopted Julian year; and suppose

Julian year (Bessel) $=(1+x)$ adopted year.

,, (Hansen)$=(1+x')$,,

Then in one adopted year

$$\text{Rotation (Bessel)} = \frac{H + L - \varpi}{1 + x}$$

$$\text{,, (Hansen)} = \frac{H' + L' - \varpi'}{1 + x'}.$$

And we thus have

$$\frac{1 + x'}{1 + x} = \frac{H' + L' - \varpi'}{H + L - \varpi}, \; = 1 + \frac{H' - H + L' - L - (\varpi' - \varpi)}{H + L - \varpi}.$$

Here

$$H' = H, \; = 365{\cdot}25 \times 360^\circ = 131490^\circ, \; = 473364000'', \; H + L = 474660027,$$
$$= \text{about } 366L:$$

hence we have

$$\frac{1 + x'}{1 + x} = 1 + \frac{L' - L - (\varpi' - \varpi)}{366L}.$$

Or neglecting as before $\varpi' - \varpi$, the ratio is

$$= 1 + \frac{0''{\cdot}055871}{366L};$$

viz. the alteration of ratio is $= \frac{1}{366}$ of the value found by Mr. Stone. As regards the term $\varpi' - \varpi$, Bessel's precession is $50''{\cdot}21129$, Hansen's $50''{\cdot}224$, whence

$$\varpi' - \varpi = 0''{\cdot}01271 \; (1 - \cos 23^\circ \; 28') = 0''{\cdot}01271 \times {\cdot}0898, \; = 0''{\cdot}00114;$$

so that instead of $0''{\cdot}055871$ the numerator should be $0''{\cdot}05473$.

The Orbit of the Great Comet (*b*) 1882. By J. Morrison, M.A., M.D., Assistant on the *American Ephemeris and Nautical Almanac*, Washington.

(*Communicated by the Foreign Secretary.*)

Ever since the discovery of this remarkable comet in the early part of September 1882, numerous efforts to determine the elements of its orbit have been made by astronomers in different parts of the world, and in no similar case has there been, we believe, a greater disparity in the results obtained. An orbit like the one under consideration, which is so situated with respect to the orbits of the planets that the effect of planetary perturbations is absolutely inappreciable, must furnish a good example of pure and undisturbed elliptic motion, and therefore we would naturally expect that a system of elements computed from three or four good observations suitably selected would very nearly, if not exactly, represent its motion. In this expectation, however, we are disappointed, for no orbit has hitherto been published which completely satisfies the observations, especially those made after the middle of October or thereabouts. From the time of discovery until about this date the nucleus

presented a tolerably well-defined circular disk which could be as easily observed as that of a planetoid; but soon afterwards extraordinary changes began to take place in the structure of the nucleus, which, from being circular in outline, became oval or elliptical, with from two to four or five bright points or centres of condensation. Thus, from about October 20 until May 26 the length of the nucleus varied from 57″ to about 135″, and the position-angle of its major axis varied from 113° to 255° —which is nearly at right angles to the meridian—hence it is not difficult to see that an error of 30″, or even more, in R.A., may exist in any observation made about this time, according to the point of the nucleus observed. The error in Declination will be much less than that in R.A. for the reason given above. The following two observations made at the Litchfield Observatory, Clinton, N.Y., and at the Naval Observatory, Washington, are here adduced to show the inconsistency of the results obtained when different points of the nucleus were observed :—

Washington Mean Time.	R.A.	Dec.
	h m s	° ′ ″
1882, Oct. 24·712191	10 6 25·78	−17 6 48·2
24·739461	10 5 52·29	−17 5 6·8

According to the geocentric motion of the comet at that time these observations are inconsistent with each other, and therefore of little or no value in the present investigation. That the discrepancy is due to this cause alone is evident from the fact there were no errors in the position of the comparison star or in the reduction of the observations, which were re-examined by the observers. It is therefore not at all suprising that periods differing very widely from each other have been obtained by different computers, according to the observations employed. Thus, from post-perihelion observations Mr. Chandler obtained periods of 3115 days and 4070 years respectively, and between these extremes we have periods of 269, 652, 712, 793, 843, 997, and 1376 years. The most probable period is between 712 and 793 years, as will appear presently. With the view of comparing the elements obtained from observations made after perihelion passage with those deduced from observations made before and after that epoch, we select the following observations :

	Washington Mean Time.	R.A.	Dec.	Where made.
	d	c ′ ″	° ′ ″	
1	1882, Sept. 8·113405	144 16 52·5	− 0 57 46·4	Windsor
2	8·452672	144 59 51·4	− 0 56 30·0	The Cape
3*	14·348690	161 28 20·0	+ 0 4 12·9	Melbourne
4*	16·368673	170 39 26·3	+ 1 12 4·8	„
5	16·936700	Ingress on Sun's disk.		The Cape
6*	19·969786	168 34 57·6	− 0 34 28·5	Washington
7	Oct. 8·720457	157 1 37·9	−10 40 21·9	„

	Washington Mean Time.		R.A.	Dec.	Where made.
		d	h m s	° ′ ″	
8		27·710103	150 31 33·9	−18 14 31·1	Clinton
9*	Nov.	15·740418	141 57 40·8	−24 49 18·9	Washington
10*		24·700923	136 34 3·3	−27 21 26·7	„
11*	Dec.	11·619921	124 5 25·3	−30 16 28·1	„
12	1883, Jan.	13·778694	100 3 54·9	−26 48 39·5	Windsor
13	Feb.	28·304652	87 56 18·0	−15 16 7·1	Washington
14	Apr.	6·097128	89 31 28·9	− 9 4 49·2	Palermo
15	May	6·252250	93 32 12·6	− 6 21 30·3	Cordoba
16		26·257880	97 11 12·6	− 5 30 0·0	„

These observations embrace nearly the entire period of visibility of the comet. No. 7 was made with the equatorial, the comparison star being a well-known one, whose position was subsequently verified by the Transit Circle; No. 8 is marked by the observer as "very good;" in Nos. 9 and 11 the "main point of condensation near the following end of the nucleus was observed," and in 13 "the middle point between the second and third points of the nucleus." No remarks respecting the remaining observations are made by the respective observers.

From Nos. 6, 7, and 13 we have obtained the following system of elements:—

I.

T	1882, Sept. 17·00858 Washington M. T.
ω	69° 37′ 27″·7 Mean Equinox of 1882·0.
Ω	346 1 38·8
i	38 0 7·7
log. q	7·8922566
log. e	9·9999660
log. a	1·9992346
P	997·36 years. Motion retrograde.

These elements, although computed with great care, do not represent the motion in the neighbourhood of perihelion. The residuals for the middle place are much greater than what would likely be due to the unavoidable errors of computation and observation. Among quite a number of places computed the smallest residuals obtained were the following:—

		$\Delta\lambda \cos\beta$	$\Delta\beta$
		″	″
3	Sept. 14·34+	+27·21	+17·20
9	Nov. 15·74+	−24·49	+12·53

It is very probable that a slight error in the third place has to some extent vitiated these results. The interval between the first and third observations is 161·3247 days, during which time the comet described an arc of only 16° 12′·1, an amount of motion far too small to permit of an accurate determination of

* Meridian observations.

the elements, especially in orbits like the present one, whose eccentricity is very large.

For the computation of a second system of elements, we will take observations which give a large amount of motion, and therefore select the three meridian observations of September 14, September 19, and November 15; the last being the first meridian observation made after the comet had ceased to be visible in daylight, and had passed from the immediate vicinity of the Sun. At this time, too, the process of disintegration in the nucleus had not advanced very far, and from the remark appended to the observation it is very probable that it is very nearly as accurate as either of the other two about which there is no doubt.

In order to show the degree of accuracy to which the computation has been carried we here give the results of the last two hypotheses. The notation is the same as that used by Gauss in his *Theoria Motus*, with the exception that we have employed u to denote the argument of latitude and ρ the curtate distance. The ratio of the sector to the triangle corresponding to the first and second observations, and also to the first and third, is *negative* and numerically less than unity.

	I.	II.
log. P	9·5492503*n*	9·5492433*n*
log. Q	8·4458408*n*	8·4458447*n*
$\omega+\sigma$	2° 25′ 49″·788	2° 25′ 50″·016
log. $Qc \sin\omega$	0·3691234	0·3691230
z	48° 1′ 1″·943	48° 1′ 1″·936
log. r'	9·3402558	9·3402558
log. ρ'	0·0539758	0·0539758
log. ρ	9·9972618	9·9972631
log. ρ''	0·0734265	0·0734271
log. r	9·3097846	9·3097852
log. r''	0·2226261	0·2226265
☊	346° 0′ 41″·385	346° 0′ 41″·254
i	38 0 7·03	38 0 7·19
$\frac{1}{2}(u''-u)$	164 52 45·703	164 52 47·13
$\frac{1}{2}(u'-u)$	157 55 32·977	157 55 34·27
$\frac{1}{2}(u''-u')$	6 57 12·726	6 57 12·86
log. η	0·1412227	0·1412226
log. η''	9·5876330*n*	9·5876319*n*
log. P_1	9·5492420*n*	9·5492433*n*
log. Q_1	8·4458464*n*	8·4458450*n*
x	+0·0000083	0·0000000
y	−0·0000056	−0·0000003

The three values of log p, the semi-parameter, are each equal to 8·1905156, which proves the accuracy of the entire computation up to this point. On completing the computation of the elements from these results, we have the following system :—

II.

T	1882, Sept. 17·010163 Washington M.T.
ϖ	69° 35′ 30″·483 Mean Equinox of 1882·0.
Ω	346 0 41·254
i	38 0 7·19
log. q	7·8895067
log. e	9·9999578
log. a	1·9016841
P	712·10 years. Motion retrograde.

The residuals for the middle place are zero. It now remains to be seen how closely these elements satisfy the above observations.

The residuals are given in the following table, where C—O denotes, as usual, the difference between the *computed* and the *observed* places, and Δ the distance from the earth.

C—O.

	$d\lambda \cos\beta$	$d\beta$	log. Δ
1	− 3″·71	− 0″·28	0·0604274
2	− 0·75	+ 1·02	0·0569198
4	+ 1·60	+ 0·82	9·9900265
7	− 0·16	− 0·44	0·1406414
8	+ 1·01	+ 0·37	0·1662759
10	+ 0·92	− 1·12	0·1766315
11	+13·41	+ 6·45	0·1916736
12	−61·20	+37·52	0·2798283
13	−45·68	+ 3·82	0·4629200
14	−52·12	−27·24	0·5880931
15	−64·07	−31·20	0·6640691
16	−54·12	−20·74	0·7022010

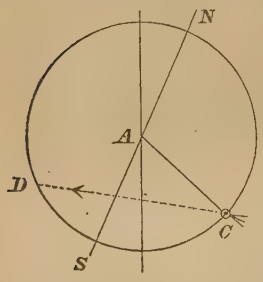
Transit over the Sun's Disk.

The ingress on the Sun's disk took place on September $17^d\ 4^h\ 50^m\ 58^s$, Cape mean time, according to Mr. Finlay, and at $4^h\ 50^m\ 52^s$, according to Dr. Elkin (*Monthly Notices*, November 1882). Adopting the former and applying the correction for aberration, which is $-0^d·005698$, we have for the position of the comet the following results :—

Washington Mean Time.	λ	β	log. Δ
1882, Sept. 16·931001	174° 27′ 19″·83	−0° 13′ 12″·39	9·9952130
Sun's position, ⊙ ...	174 36 19·06	−0 0 0·22	
Difference ...	0 8 59·23	13 12·17	

Distance between the centres	15′ 58″·28
Sun's semi-diameter	15 57·73
Comet's distance from Sun's limb ...	0·55

The diagram shows the position of the comet with respect to the Sun at the time of ingress.



N S is a meridian; C D the apparent path of the comet; N A C the angle of position, 122° 23′·7 from the north point of the Sun's limb towards the west.

Since this second system of elements represents the motion of the comet with great accuracy from the time of discovery till about the middle of November, and from this date until the last observation here recorded, with all the precision that can be expected under the circumstances, we may also give the following facts as tending to show more strongly perhaps the characteristics of this remarkable body.

At the time of ingress on the Sun's disk the comet's true anomaly was 92° 7′ 29″·668, which was described in *one hour and fifty-four minutes*. From the time of the first observation made at Windsor by Mr. Tebbutt until May 26th, an interval of 260·1447 days, the comet described 340° 22′·7 of its orbit, but it will require more than seven centuries to describe the remaining 19° 37′·3. Taking the mean solar parallax at 8″·848, the value adopted in the English and American *Nautical Almanacs*, the comet's perihelion distance was 716200 miles from the Sun's centre, or 285500 miles from the surface.

The velocity at perihelion was 295·36 miles per second, which is only ·0160 miles per second less than that due to a parabola at the same perihelion distance.

When the comet reaches aphelion, it will have plunged down into space a distance equal to 160 radii of the Earth's orbit, or $5\frac{1}{2}$ times the radius of *Neptune's* orbit, at which enormous distance its velocity will be only 75 feet per second.

At the time of Dr. Elkin's first observation on Sept. 8, the apparent diameter of the coma was estimated at from 40″ to 50″, and the strongly condensed cone or nucleus in the centre at from 10″ to 15″. Assuming the means of these measurements,

we easily find the diameter of the coma at that time to have been 22500 miles, and of the nucleus 7600 miles, or a little less than that of the Earth. As the comet approached the Sun, the nucleus contracted, and about half an hour before its disappearance on the solar disk Mr. Finlay found by two micrometric measures the apparent diameter to be 4″, while Dr. Elkin estimated it at some 5″. The former of these gives a diameter of 1770 miles, and the latter 2215 miles, or a little greater than that of the Moon.

The second system of elements here presented proves conclusively that an orbit can be found which satisfies the motion of the body equally well both before and after peribelion; and therefore we may safely conclude that no retardation occurred during the perihelion passage; or, if there was, it is absolutely inappreciable. There are two other systems of elements which differ considerably in the periodic time from our second system, but the agreement of some of the other elements is very remarkable. We refer to those of Prof. Frisby and Dr. Kreutz; the former founded his calculations on the observations of Sept. 19, Oct. 8, and Nov. 24, and the latter on normal places derived from observations extending from Sept. 8 to Nov. 15. The three systems just referred to are here given for comparison.

	Dr. Morrison.	Prof. Frisby.	Dr. Kreutz.
T*	1882, Sept. 17·010163	1882, Sept. 17·0088	1882, Sept. 17·00993
ω†	69° 35′ 30″·48	69° 36′ 12″·79	69° 36′ 1″·50
Ω	346 0 41·25	346 1 7·91	346 1 27·20
i	38 0 7·19	38 0 7·84	38 0 19·9
log. q	7·8895067	7·8904739	7·8894760
log. e	9·9999578	9·9999606	9·9999610
log. a	1·9016841	1·9331366	1·9505596
P	712·10 yrs.	793·869 yrs.	843·10 yrs.
	Mean Equinox 1882·0	Mean Equinox 1882·0	Mean Equinox 1882·0

* Washington Mean Time. † Motion retrograde.

This comet is probably identical with the comet of 370 B.C., as already pointed out by Mr. Maxwell Hall. If we divide the interval between 370 B.C. and 1882 A.D. by three we get a period of 751 years, which is very nearly the arithmetic mean between our period and that of Prof. Frisby. This period would give a return in A.D. 1131 or 1132, in both of which years a great comet appeared (*Chinese Annals*).

Observations of Pons's Comet made at the U.S. Naval Observatory, Washington, with the 9-inch Equatorial.

By Commander W. T. Sampson and Prof. E. Frisby.

(*Communicated by the Secretaries.*)

1883.	Washington M.T.	Comet−✶ Δα	Comet−✶ Δδ	No. of Comp.	Comet α app.	log. (p×Δ)	Comet δ app.	log. (p×Δ)	Obs.	Comp. Star.
	h m s	m s	′ ″		h m s		° ′ ″			
Sept. 23	8 8 24·5	+0 1·19	− 2 10·3	15·3	16 25 20·93	9·89564	+60 31 56·8	9·72026*n*	S	1
24	11 19 47·3	+4 57·92	− 2 12·9	15·3	16 25 28·49	9·94552	60 16 47·8	0·76095	S	2
26	7 42 33·3	− 31·98	−10 10·6	25·5	16 25 47·71	9·87342	59 52 22·8	9·82982*n*	S	3
26	9 56 0·3	+0 16·86	+ 7 7·4	18·4	16 25 47·07	9·96138	59 51 16·0	0·41280	F*	4
28	8 9 12·4	−2 4·47	− 9 35·9	20·3	16 26 17·22	9·91121	59 26 16·6	9·38115	F	5
30	9 39 24·7	+3 32·05	− 5 24·4,	5·1	16 27 5·26	9·94484	58 58 54·6	0·57845	S	6
30	9 50 37·1	+3 32·02	− 5 12·4	5·1	16 27 5·23		58 58 42·6		S	6
Oct. 2	9 30 9·2	+1 39·78	+ 3 46·8	25·5	16 28 2·02	9·30293	58 33 52·3	9·51845*n*	S	7
2	9 30 9·2	+3 12·29	+ 3 19·9	25·5	16 28 2·41		58 33 51·8		S	7
8	7 43 21·6	−0 48·60	+ 6 4·3	25·5	16 32 2·33	9·88741	57 16 55·5	9·73798	S	9
9	7 17 34·7	0 0·00	− 6 27·7	10·2	16 32 50·90	9·87016	57 4 23·3	9·39904	S	9
9	7 24 12·3	0 0·00	− 6 31·6	10·2	16 32 50·90		57 4 19·4		S	9
10	7 22 41·5	−9 20·48	− 7 57·0	15·3	16 33 43·74	9·86011	56 51 49·0	7·65475	S	10
11	7 54 46·7	+8 5·37	−12 44·3	15·3	16 34 39·90	9·89986	56 38 56·5	0·09116	S	11
11	8 6 16·5	+8 6·21	−12 50·3	10·2	16 34 40·74		56 38 50·5		S	11
19	9 3 14·1	−6 37·23	+ 4 45·5	10·2	16 43 45·76	9·90472	55 1 19·7	0·48745	F	12
24	6 52 24·8	+2 7·06	+ 5 20·8	15·3	16 50 49·66	9·84559	54 4 23·8	9·87235	S	13

* Made with 26-in. Equatorial,

1883.	Washington M.T. h m s	Comet − ✱ Δα m s	Δδ ′ ″	No. of Comp.	Comet α app. h m s	log. ($p \times \Delta$)	Comet δ app. ° ′ ″	log. ($p \times \Delta$)	Obs.	Comp. Star.
Oct. 24	7 9 40·5	+1 58·15	− 9 57·3	15·3	16 50 52·28	9·85554	+54 3 58·0	9·99734	S	14
24	7 43 13·8	+2 8·19	− 6 51·0	15·3	16 50 55·44	9·88111	54 3 43·4	0·28026	S	15
26	7 9 43·0	−4 36·67	−10 50·6	25·5	16 54 4·86	9·85996	53 41 9·2	9·51435	S	16
27	7 24 50·3	−5 30·99	+ 6 1·3	20·4	16 55 47·79	9·73856	53 29 48·1	0·25273	S	17
29	9 10 27·2	+2 2·91	− 6 46·3	25·5	16 59 27·40	9·87621	53 6 25·0	0·63868	S	18
30	6 45 21·5	−3 36·47	+ 6 10·5	20·4	17 1 5·93		52 56 31·7		F	19
30	6 45 21·5	−5 5·84	+ 9 20·4	11·3	17 1 5·68	9·83976	52 56 29·7	0·00232	F	20
30	6 45 21·5	−5 28·43	+ 3 15·3	10·4	17 1 6·65		52 56 31·1		F	21
30	7 27 59·8	+2 38·29	+ 9 33·3	15·3	17 1 10·82		52 56 12·9		S	22
31	7 20 16·9	−1 38·18	− 5 15·5	15·2	17 3 4·19		52 45 5·6		F	19
31	7 20 16·9	−3 7·77	− 2 58·9	14·2	17 3 3·73	9·86293	52 45 10·3	0·27512	F	20
31	7 20 16·9	−3 31·13	− 8 10·2	9·2	17 3 4·11		52 45 5·5		F	21
31	7 49 19·2	+4 32·39	− 1 50·6	10·2	17 3 4·89		52 44 48·8		S	22
Nov. 1	8 13 14·8	+2 24·60	+ 2 45·2	20·4	17 5 4·43	9·87821	52 33 42·4	0·50105	S	23
1	8 13 14·8	−0 11·65	+ 5 17·2	20·4	17 5 4·80	9·87821	52 33 42·2	0·50108	S	24
3	8 23 28·3	−1 46·64	−14 59·7	20·4	17 9 10·03	9·87485	52 11 33·5	0·54866	S	25
4	7 11 11·0	−2 1·52	+ 5 2·2	20·4	17 11 11·40	9·85598	52 1 26·8	0·27695	S	26
5	7 12 20·8	−4 20·30	+11 38·7	15·3	17 13 22·43	9·85560	51 50 33·2	0·29402	F	27
9	7 18 43·8	−4 8·48	− 4 39·7	10·2	17 22 42·53	9·85479	51 6 47·0	0·36871	S	28
9	7 18 43·8	−3 25·84	− 8 53·8	10·2	17 22 42·96		51 6 51·2		S	29

1883.	Washington M.T. h m s	Comet−✶ Δα m s	Δδ ′ ″	No. of Comp.	Comet α app. h m s	log. (p×Δ)	Comet δ app. ° ′ ″	log. (p×Δ)	Obs.	Comp. Star.
Nov. 10	7 27 47·3	−0 55·61	− 2 7·7	15·3	17 25 13·17	9·85690	+50 55 49·4	0·41411	S	29
12	7 10 48·8	+1 1·15	+ 5 33·5	25·5	17 30 21·15	9·85098	50 34 15·1	0·39056	F	3
12	7 21 47·4	+0 1·88	− 2 42·0	15·3	17 30 22·67	9·85208	50 33 58·0	0·40286	F	31
12	7 48 38·8	+3 41·21	− 6 15·3	15·3	17 30 26·64	9·85862	50 33 48·3	0·50127	S	32
14	5 51 28·4	−2 25·86	+ 9 0·5	20·4	17 35 41·97	9·78291	50 12 41·5	9·84613	S	33
14	7 7 34·3	−6 11·46	+ 6 22·0	15·3	17 35 50·59	9·84441	50 12 8·6	0·36478	S	34
14	7 7 34·3	−4 58·75	+ 5 35·9	15·3	17 35 51·56	9·84439	50 12 7·2	0·36473	S	35
15	5 49 33·9	−3 31·09	− 4 25·0	15·3	17 38 30·96	9·78037	50 1 21·4	9·84912	S	34
15	5 49 33·9	−2 18·38	− 5 7·4	15·3	17 38 31·91	9·78035	50 1 23·4	9·84888	S	35
15	7 23 55·6	+0 33·66	− 3 1·3	20·4	17 38 41·44	9·84870	50 0 39·5	0·43837	F	33
15	7 41 19·5	−1 49·28	+ 4 27·4	15·3	17 38 44·67	9·85316	50 0 35·2	0·51666	F	36
16	6 26 11·8	+0 54·84	− 6 21·1	15·3	17 41 28·77	9·81964	49 49 46·5	0·19851	F	36
16	6 36 23·5	−2 2·92	+ 2 25·3	15·3	17 41 31·80	9·82423	49 49 49·5	0·23341	S	37
17	6 33 45·4	+0 56·44	− 9 11·0	15·3	17 44 31·15	9·82128	49 38 12·9	0·22918	S	37
18	7 32 18·0	−1 11·16	+ 7 51·0	20·4	17 47 43·11	9·84687	49 26 16·6	0·48177	S	38
19	5 51 54·6	+1 45·09	− 3 13·3	20·4	17 50 39·35	9·78054	49 15 11·8	9·95965	S	38
19	6 9 53·1	−2 56·69	+ 4 50·4	20·4	17 50 41·19	9·79928	49 15 2·6	0·10411	S	39
27	5 46 59·1	+0 3·95	− 0 25·5	25·5	18 19 6·02	9·76351	47 30 24·8	0·03048	S	40
27	6 1 7·5	+0 5·98	− 0 28·5	20·4	18 19 7·93	9·77887	47 30 21·9	0·12947	F	40
27	7 13 17·8	+0 18·39	− 1 22·4	20·4	18 19 20·45	9·82614	47 29 27·8	0·44808	S	40
28	5 44 1·7	−0 43·54	− 2 31·1	25·5	18 23 6·27	9·75792	47 15 38·8	0·01931	S	41
30	7 34 31·4	−2 59·98	− 0 4·6	25·5	18 31 45·94	9·82085	46 42 40·7	0·52792	F	42

Assumed Mean Places of Comparison Stars.

Star.	α 1883·0.	δ 1883·0.	Authority.
	h m s	° ′ ″	
1	16 23 13·67	+60 34 2·4	Bonn
2	16 20 31·19	60 18 43·4	Lal. Fed.
3	16 26 20·28	60 2 15·8	$\frac{\text{O. Arg. N.} + \text{Bonn}}{2}$
4	16 25 30·79	59 43 51·1	Bonn
5	16 28 22·29	59 35 35·0	O. Arg. N.
6	16 23 33·87	59 3 38·4	„
7	16 26 22·88	58 29 48·8	„
8	16 24 50·77	58 30 15·4	„
9	16 32 51·57	57 10 34·8	„
10	16 43 4·80	56 59 28·5	Berliner Jahrbuch
11	16 26 35·40	56 51 39·4	O. Arg. N.
12	16 50 23·58	54 56 17·7	„
13	16 48 43·22	53 58 48·0	„
14	16 48 54·77	54 13 40·2	„
15	16 48 47·89	54 10 17·4	„
16	16 58 42·12	53 51 43·9	„
17	17 1 19·40	53 23 31·4	„
18	16 57 25·10	53 12 56·3	Bonn
19	17 4 42·93	52 50 5·4	$\frac{\text{Bonn} + \text{Rümker}}{2}$
20	17 6 12·05	52 46 53·3	Bonn
21	17 6 35·62	52 52 59·7	„
22	16 58 33·10	52 46 24·6	O. Arg. N.
23	17 2 40·37	52 30 42·1	Bonn
24	17 5 16·99	52 28 9·5	O. Arg. N.
25	17 10 57·22	52 26 17·3	„
26	17 13 13·41	51 56 8·7	Lalande
27	17 17 43·18	51 38 38·2	Bonn
28	17 26 51·44	51 11 10·3	O. Arg. N.
29	17 26 9·21	50 57 41·0	Struve
30	17 29 20·40	50 28 25·5	Bonn
31	17 30 21·19	50 36 23·8	„
32	17 26 45·86	50 39 47·9	O. Arg. N.
33*	17 38 8·16	50 3 24·2	„

* Argelander's Declination for this star has been increased by 1 micrometer rev. = 47″·2.

Star.	α 1883·0. h m s	δ 1883·0. ° ′ ″	Authority.
34	17 42 2·36	+50 5 29·1	O. Arg. N.
35	17 40 50·62	50 6 13·7	,,
36	17 40 34·25	49 55 50·8	,,
37	17 43 35·02	49 47 6·9	,,
38	17 48 54·51	49 18 8·0	,,
39	17 53 38·09	49 9 54·4	Bonn
40	18 19 2·06	47 30 31·0	O. Arg. N.
41	18 23 49·77	47 17 50·2	,,
42	18 34 45·80	46 42 24·6	,,

Comparison of Observations with Ephemeris.

Date.	Obs.—Comp. Δα s	Δδ ″	Date.	Obs.—Comp. Δα s	Δδ ″	Date.	Obs.—Comp. Δα s	Δδ ″
Nov. 1	−0·06	+20·0	Nov. 12	+1·60	+13·6	Nov. 17	+2·62	+14·7
,, 1	+0·26	+19·8	,, 14	+2·76	+16·7	,, 18	+2·82	+25·9
,, 3	+0·26	+11·4	,, 14	+1·14	+16·1	,, 19	+2·58	+18·7
,, 4	+0·73	+26·9	,, 14	+2·75	+24·5	,, 19	+2·16	+17·9
,, 5	+0·94	+27·5	,, 15	+1·90	+13·4	,, 27	+4·11	+ 9·4
,, 9	+0·96	+14·8	,, 15	+3·72	+28·8	,, 27	+3·72	+15·3
,, 9	+1·48	+17·6	,, 15	+3·15	+15·4	,, 27	+4·57	+ 6·6
,, 10	+1·20	+12·6	,, 15	+2·43	+20·7	,, 28	+4·03	+15·6
,, 12	+0·32	+24·1	,, 16	+0·61	+16·4	,, 30	+5·57	+ 9·3
,, 12	+0·60	+12·0	,, 16	+2·39	+23·6			

Unfavourable weather prevented the comet being seen before Sept. 23. On that evening I was surprised at its brightness, as it had been described as a faint object. The light was a bright yellow, without showing any decided nucleus. On the 24th the appearance was much the same as on the previous evening. On the 25th it was not seen. On the 26th its appearance had changed in a most marked manner. It was much fainter, and the light had lost its yellow colour.

Observations of Comet b, 1883 (*Pons-Brooks*), *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The observations were made with the East or Sheepshanks Equatorial (aperture 6·7 inches) by taking transits over two cross-wires at right angles to each other, and each inclined 45° to the parallel of declination.

Gr. Mean Solar Time. 1883, Nov.	♂−✶ R.A.	Corr. for Par. and Refrac. in R.A.	♂−✶ N.P.D.	Corr. for Par. and Refrac. in N.P.D.	No. of Comp.	App. R.A.	App. N.P.D.	
			Observer: AD.					
d h m	m s	s	′ ″	″		h m s	° ′ ″	
10 6 52	+3 4·61	+0·36	−11 2·0	−2·2	2	17 24 37·61	39 0 48·8	*a*
7 1	−1 31·12	+0·38	− 0 21·0	−2·1	2	17 24 38·01	39 1 3·4	*b*
			Observer: T.					
17 7 7	+0 52·69	+0·39	− 6 30·4	−2·5	5	17 43 55·43	40 19 54·6	*c*
7 5	−0 22·86	+0·39	+ 7 1·8	−2·4	2			*d*
			Observer: H.					
19 6 58	+1 13·66	+0·41	+ 1 32·7	−2·6	1	17 50 7·95	40 43 6·2	*e*
9 10	+1 29·65	+0·40	+ 2 15·9	−4·4	6	17 50 23·94	40 43 49·4	*e*
9 16	−3 14·86	+0·39	− 5 52·4	−4·2	3	17 50 23·06	40 43 53·8	
			Observer. T.					
27 5 56	+0 16·30	+0·40	− 2 47·3	−2·3	5	18 18 17·38	42 25 39·9	*g*
6 8	−0 42·35	+0·43	− 2 39·9	−2·5	9	18 18 18·16	42 26 38·7	*h*

Assumed Mean Places of the Comparison Stars.

Star.	Star's Name.	R.A. 1883·0.	N.P.D. 1883·0.	Authority.
		h m s	° ′ ″	
a	Lalande 31840	17 21 33·44	39 12 6·2	Lalande
b	Arg. Z. + 50° − 2412	17 26 9·55	39 1 40·6	Bonn. Obs., vol. v.
c	Arg. Z. + 49° − 2689	17 43 3·02	40 26 41·7	„
d	Anonymous			
e	Oeltz. Arg. (N) 17595	17 48 54·55	40 41 50·6	Oeltz. Arg. (N) 1842
f	Arg. Z. + 49° − 2714	17 53 38·13	40 50 4·0	Bonn Obs., vol. v.
g	Arg. Z. + 47° − 2621	18 18 1·08	42 28 46·3	„
h	Arg. Z. + 47° − 2625	18 19 0·50	42 29 37·8	„

Nov. 10. The comet very faint; bright moonlight. The star *a* of the 8th—9th mag.; *b* of the 9th magnitude.

Nov. 17. Comet faint and diffused.

Nov. 27. Comet very faint and diffused.

The observations are corrected for parallax and refraction.

The initials AD., T., and H. are those of Mr. Downing, Mr. Thackeray, and Mr. Hollis.

Royal Observatory, Greenwich:
1883, *Dec.* 14.

On the Spectrum of Comet b 1883 (*Pons-Brooks*), *observed at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The spectrum of this comet has been examined by Mr. Maunder on the nights of Nov. 27 and Dec. 6 with the "half-prism" spectroscope mounted on the Great Equatorial. The spectroscope was used in the "reversed" position, as in the observations of prominences, so as to give great purity of spectrum. The dispersion from A to H in this position is 5°, and the magnifying power employed was 14.

On Nov. 27 the observations were much interrupted by the passage of light cloud, and no measures were obtained. The spectrum consisted of the three usual cometary bands, with edges bright and sharp towards the red, and fading gradually away towards the blue. No continuous spectrum could be certainly made out, though the band in the yellow, which was very faint, could be traced nearly up to the sharp edge of the band in the green, and that again could be traced very nearly up to the sharp edge of the band in the blue. The bands in the red and violet were not looked for. The band in the green was fairly bright, and the edge towards the red sharp and well defined. The band in the blue was much fainter, and its less refrangible edge not so well defined as that of the band in the green. The band in the yellow was fainter even than that in the blue.

As seen in the finder, the comet appeared nearly circular, about 4′ or 5′ in diameter; it showed a central condensation of stellar appearance, and a faint prolongation of the comet at about 20° position-angle seemed to indicate the commencement of a tail.

On Dec. 6 the comet was decidedly brighter than on Nov. 27, but its appearance as seen in the finder had not undergone any material change. The spectrum, however, seemed somewhat different. There was a very distinct continuous spectrum about 1″ in breadth, and a fainter continuous spectrum was suspected to extend for 25″ or 30″ on both sides of this bright central spectrum. The band in the green was traced for a total length of 4′, and probably extended further still. The band in the yellow was traced for a total length of about 2′ or 2′ 10″; the band in the blue for about 70″ or 80″. The band in the yellow was now very much brighter than that in the blue; the intensities of the three bands being roughly estimated as follows:—Yellow band 5. Green band 10. Blue band 1. The red and violet bands were not looked for. The less refrangible edges of all three bands were very sharp and well defined.

The following measures were obtained of the sharp edge of the green band of the comet spectrum as compared with the

green band of the spectrum given by a vacuum tube containing alcohol.

Greenwich Sidereal Time.	Comet-band—Alcohol band		Inferred Wave-length of Comet band.
	Micrometer Reading	Wave-length tenth-metres	
h m	r		
0 30	1·885	34·5	5162·5
0 45	2·269	41·5	5155·5
1 5	2·176	39·8	5157·2
1 20	1·980	36·2	5160·8
1 40	1·708	31·3	5165·7
		Mean	5160·3

The green band of the comet spectrum was obviously not coincident with the green band of the alcohol spectrum, but far to the blue of it. There was, however, a bright line in the spectrum from the alcohol vacuum tube coincident with the green band of the Bunsen-flame, and with this the comet-band was very nearly coincident. It seemed, however, a little to the blue of it. The width of the slit during the above measures was 15·8 tenth-metres.

Thalén's wave-length for the less refrangible edge of the green band in the spectrum of the alcohol was assumed throughout: viz. 5197·0 tenth-metres.

Royal Observatory, Greenwich:
1883, *Dec.* 13.

Rotation Period of Jupiter. By W. F. Denning.

Observations of the last few years have proved that one rotation period will not satisfy the motion of different markings on the surface of *Jupiter*. Nor will one period satisfy the same marking for a lengthened interval, as the individual spots have in certain prominent cases shown a slackening motion, the amount of which increases with the time.

At Bristol since 1880, September 27, the great red spot has been observed as it crossed the planet's central meridian on 152 nights. The equatorial white spot has been similarly recorded on 150 nights. Other markings of apparently less permanent character have been carefully watched for shorter intervals. I have partly derived the longitudes of the red and white spots and otherwise reduced the observations by the aid of Mr. Marth's valuable Ephemerides published in the annual volumes of the *Monthly Notices*.

As to the red spot, the following observations have been obtained during the present apparition:—

	On C. M.	Longitude.
	h m	°
1883, Aug. 23	16 48	67·3
Oct. 15	15 37	71·7
17	17 14	71·1
20	14 44	71·6
Nov. 3	16 17	73·8
18	13 34	72·1
19	19 25	74·8
25	14 25	76·3
29	17 38	75·0
Dec. 4	16 48	77·4
5	12 41	78·6

The observed times of the passages across the central meridian of *Jupiter* are in every case estimated. This method, though undoubtedly less accurate than direct measurement, is found to be very reliable, for the apparent motion of an object near the central meridian is so great that an interval of 3^m is sufficient to introduce a change of place obvious to the eye. It is believed therefore, that in all the observations where the conditions of seeing have been favourable the average error is less than 2^m. The longitudes have been computed from the daily rate of $870°{\cdot}42$ = rotation of $9^h\,55^m\,34^s{\cdot}47$, which Mr. Marth has adopted for his recent Ephemerides.

The spot shows a very decided increase in its rotation period. My results for the last few oppositions are as follow:—

Interval.	Number of Rotations.	Period of Rotation.
		h m s
1880, Sept. 27–1881, Mar. 17	413	9 55 35·6
1881, July 8–1882, Mar. 30	640	9 55 38·2
1882, July 29–1883, May 4	674	9 55 39·1
1883, Aug. 23–1883, Dec. 5	251	9 55 38·8

Comparing my observation on 1883, December 5, $12^h\,41^m$, with an observation made by Mr. Trouvelot at Cambridge, Mass., on 1878 September 25, 6^h55^m, Cambridge mean time, I find that the average rotation period of the spot has been

h m
9 55 36·2

during the 1897 days (=4586 rotations) embraced by the observations.

Professor Hough gives the period as $9^h\ 55^m\ 35^s{\cdot}9$ for the interval 1879, September 25–1882, March 29, comprising 916 days (2214 rotations). He notes that in 1879 the period was $9^h\ 55^m\ 34^s{\cdot}0$, while during the last opposition it had increased to $9^h\ 55^m\ 38^s{\cdot}4$. These results are in very good accordance with those obtained at Bristol.

At the present time the spot follows the assumed first meridian of *Jupiter* (*Monthly Notices*, June 1883) by about $2^h\ 10^m$, and the interval is increasing at the rate of 4^m per month.

During the opposition of 1882–3 the spot became extremely faint, so that when the planet left us in May 1883 it was felt that, should the spot continue to fade, it must cease to be visible by the time *Jupiter* reappeared from conjunction at the end of August. But the spot was re-detected by Mr. A. S. Williams at Brighton, and by myself at Bristol, on the morning of August 24, and since then has been distinguished by several other observers, though Signor Ricco, of the Palermo Observatory, failed to find any traces of the spot on the mornings of September 11, 14, and 21, and announced its disappearance.* With the 10-inch Refractor of that Observatory the region of the disk formerly occupied by the spot appeared very white. Mr. Williams, however, saw the spot well as it came to the central meridian of *Jupiter* on September 14 and 16, and during the ensuing months of October and November it has been very plainly visible. On December 4, as it crossed the central meridian, I saw it with great distinctness and definiteness of outline, and conclude that it is certainly as plain now as it was nine months ago. The fact that it has become more prominently visible during the last few weeks is explained by the more favourable position of *Jupiter*, and not by any actual deepening of tint in the spot itself.

It is altogether open to conjecture whether this marking is identical with certain large and definite spots which have been observed in former times, and exhibited a nearly similar period of rotation. The spots seen by Hooke and Cassini in 1664 and subsequent years cannot be held to have any certain relation to the one now visible, as there is an absence of intermediate evidence to serve as a connecting link. The old observations unmistakably refer to an object in nearly the same position as the spot of our own time, but the former must have been considerably less in magnitude, it being estimated as only one-tenth the diameter of *Jupiter*, whereas the present red spot is about one-third; in fact the latter is fully three times the size of the former.

But though it seems impossible to trace a distinct association with markings which appeared at such a remote period, we may perhaps discern a more satisfactory analogy with some modern observations. We may feel assured, however, that in the time of Lassell and Dawes there was no large and permanent spot on the

* Sig. Ricco writing to *L'Astronomie* for Dec. 1883, states that he succeeded in re-detecting the spot on Sept. 28.

surface of *Jupiter*. Had such a phenomenon presented itself it must have awakened a keen interest in observers eager to avail themselves of the investigation which such an important apparition must invite. But in 1869 and 1870 an object was discovered by Mr. Gledhill, which from its position, motion, and aspect appears to have been the precursor of the red spot. It was first seen on November 14, 1869, and "carefully observed until March 1870, but in July of the same year it disappeared," though on December 1, 1871, a similar object was observed apparently resting on the southern margin of the equatorial dark belt. The marking seen by Mr. Gledhill was in the form of a dark elliptical ring, its major axis lying parallel with the belts. It was darkest at the E. end, and appears to have occupied the same latitude as the red spot. On January 23, 1870, and other occasions, a narrow belt was noticed immediately outlying the great S. belt, and bent abruptly to the N. just under the following end of the ellipse. This is precisely what has been observed in regard to a small belt following the red spot in 1880 and 1881, and the fact of the dark E. extremity of the ellipse offers additional corroboration to the theory of its identity with the red spot, for the latter during the time I have observed it has always shown a very dark minute spot at its E. end. Moreover, the rotation period of the ellipse during the winter months of 1869–70 was about $9^h\,55^m\,33^s$, which nearly agrees with that of the red spot.

Should this ellipse have developed into the red spot, it is curious what became of it during the years from 1871 to 1878. Mr. Corder in March and April 1872 saw a large elliptical spot in the S. hemisphere of the planet, and Lord Rosse in the following year observed a similar object. In 1876 Mr. Russell at Sydney also mentions having seen the spot, though during the opposition of that year no corroboration is met with amongst the results of other observers.

If the ellipse of 1869–70 was really the forerunner of the red spot, the period of rotation has increased but little during the intervening ten years, and the slackening motion which has become so well defined since 1880 cannot previously have affected the spot to any great extent, unless there is an oscillatory motion of long period, which seems quite possible. In connection with this question it is interesting to note that the increasing retardation of the red spot as observed chiefly in 1881 and 1882 seems now to have become less decided. My own observations show an increase of 2·6 sec. in the rotation period, ascertained during the successive apparitions of 1880–81 and 1881–82, whereas the increase in 1882–3 over 1881–82 was only 0·9 sec. And the observations so far as they have progressed during the present apparition prove that the period has decreased 0·3 sec. as compared with the results of the previous apparition. The question naturally arises, therefore, as to whether the spot has now commenced to drift in W. longitude? If so, we shall soon realise its effects in the shortening of the rotation period. The

observations of the last few months are very suggestive in this respect.

The equatorial white spot has continued well visible during the present year, though on the whole I believe it is scarcely so brilliant as in the autumnal months of 1880. From a large number of observations obtained here since 1882 I have selected about one per month to exhibit the increasing longitude of the spot relatively to the daily rate of 878°·46=rotation of $9^h\ 50^m\ 7^s{\cdot}42$, which Mr. Marth found conformable to the observations of 1880 Oct.–1882 Aug.:—

	On C. M.	Longitude.
	h m	°
1882, July 29	15 49	29·8
Aug. 23	15 51	29·9
Sept. 27	11 52	28·9
Oct. 25	18 32	31·0
Nov. 26	17 37	31·7
Dec. 23	8 51	32·9
1883, Jan. 23	12 33	42·6
Feb. 23	6 13	41·5
Mar. 24	8 45	45·1
May 4	8 32	45·0
Aug. 24	16 35	62·8
Oct. 15	17 56	67·3
Nov. 3	14 17	64·3
25	17 25	66·2

It is evident by the marked increase of longitude, which the table shows, that this white spot moves considerably slower than during the earlier observations. For the purpose of tracing the extent of this retarded velocity, I have arranged my observations into three periods, with the following result:—

Interval.	Number of Rotations.	Period of Rotation.
		h m s
1880, Oct. 20–1881, Sept. 30	842	9 50 5·8
1881, Sept. 30–1882, Dec. 23	1095	9 50 8·8
1882, Dec. 23–1883, Nov. 25	823	9 50 11·4

The rate of increase in the rotation period during the three years included in the observations has been nearly six seconds, which is greater than the retardation in the motion of the red spot. The mean period of the white spot from the whole interval of my observations is

h m s
9 50 8·7,

which comprises 1131 days (=2760 rotations).

The conjunctions of the two spots are extremely interesting, because at such a time their great differences of velocity are strikingly manifested. In the *Monthly Notices* for Jan. 1882 I gave the interval of these conjunctions as $44^d\ 10^h\ 42^m$, and from the observations which have accumulated since that time this period is proved to be not very far from the truth. On Nov. 25, at $17^h\ 25^m$ the white spot followed the red spot by 3^h, and on Dec. 19 at $12^h\ 3^m$, the white spot preceded it by $2^h\ 6^m$, so that allowing for the diurnal gain of $13^m\ 24^s$ by the white spot, the conjunction must have occurred (from a mean of the two observations) on Dec. 9 at about 15^h. This compared with an observation on Nov. 19, 1880, at $9^h\ 23^m$, when the two markings occupied exactly the same longitude, gives the average period from 25 conjunctions which have occurred in the interim of $1115^d\ 5^h\ 37^m$ as

d	h	m
44	14	37·5.

Should the two spots remain visible, they may be observed near conjunction at the following times :—

1884.	h m	1885.	h m
Jan. 23	5 37·5	Jan. 15	2 37·5
Mar. 7	20	Feb. 28	17 15
Apr. 21	10 52·5	Apr. 13	7 52·5
June 5	1 30	May 27	22 30
July 19	16 7·5	July 11	13 7·5
Sept. 2	6 45	Aug. 25	3 45
Oct. 16	21 22·5	Oct. 8	18 22·5
Nov. 30	12 0	Nov. 22	9 0

The motion of the white spot is affected by some curious variations. Even in a few nights it occasionally shows a considerable displacement, so that the resulting longitudes vary in the most striking and apparently irregular manner. Referring to the table of longitudes on the previous page, we shall find that it increased nearly 10° during the month preceding 1883 Jan. 23, and that during the interval from 1883 May 4 to Aug. 24 the increase was nearly 18°.* But during the last few observations, up to 1883 Nov. 25, the velocity of the spot appears to have been more consistent. These singular displacements of position show irregularities both in extent and period, and have been well corroborated by other observers. Thus Prof. Hough ("Report of Dearborn Observatory, 1882," p. 50, remarks, "During 1881 the spot, observed continuously for a period of 252 days, in-

* This displacement of 18° is equivalent to more than 13,000 miles on the surface of *Jupiter*.

dicated sudden deviations in its apparent place, probably due to change of shape in the object observed." On Oct. 8, 1881, he noted "a maximum displacement of 16 minutes of time, or more than 3″ of arc at mean distance."

There are two other bright objects near the principal white spot, and it has sometimes occurred to me that the rapid displacements in its position may result from the alternate visibility of these several objects. While the chief spot is near its minimum, one of the bordering spots may assume greater prominence, and the observer will thus be easily misled. This explanation offers a satisfactory solution to the phenomena observed; but though I have critically watched the behaviour of these objects I could never be certain that this supplied the real cause of the difficulty. Could their relative appearances be noted nearly every night for a few weeks the point might doubtless be settled.

Apart from its vagaries of motion, the white spot changes its visible aspect very materially. It is sometimes extremely large and brilliant, shining with almost startling effect on the equatorial border of the great southern dark belt. At other times it is very small and faint, and only perceptible after close examination. On four different occasions I have absolutely failed to detect any vestige of the spot, though the central part of the disk was thoroughly examined at the time of its passage.

The spot also varies in form as well as in brightness. It is usually in the shape of a brilliant oval, well defined, and occasionally quite as large as the third satellite of *Jupiter*; but it is sometimes seen as a diffused white patch apparently emerging from the N. edge of the belt. Whenever the spot is bright and well defined there is a trail of light, or diffused luminous matter, running from its east side as though there were an eruption of luculent material from the spot which the rapid rotation of the planet from W. to E. caused to drift in an opposite direction.

The cause of the obscurations * to which the spot is subject would be easily explained on the supposition that the dark spots and patches which abound along the equatorial zone move with a different velocity, and temporarily overlap it. But from careful observation of the equatorial markings it is proved that their

* I may adduce as an example of these variations of light, some observations obtained here in Dec. 1882 and Jan. 1883. On Dec. 22 and 23 the spot was recorded as very brilliant, but on Jan. 1 it had become so far reduced as to be utterly invisible in my 10-inch Reflector. On Jan. 3 I re-detected it, but it was very faint. On Jan. 9 the spot was described as bright, and on the 13th had arrived at a maximum, so as to present a very conspicuous appearance on the disk of the planet. But a far more surprising change was noticed in regard to a small white spot immediately preceding the chief spot (by about 9°) in Nov. 1880. On the 17th, at $7^h\ 56^m$ this object was described as small and bright as it passed the C. M., but on the 19th it was quite obliterated. A few days later, I received a letter from Mr. F. C. Dennett of Southampton, in which he referred to the same thing, so that in this case we have a well-corroborated instance of rapid change.

velocity is identical with that of the white spot. They are seen to maintain the same relative distances, so that, whatever the cause of the obscurations in the white spot, it is certainly not due, as was formerly supposed, to the interposition of the dark spots in the same latitude. We are thus led to the inevitable conclusion that the variations of brightness are attributable to some inherent phenomena resulting from the peculiar constitution and condition of the spot itself. The appearances would indicate a volcanic nature, and its frequent increase and decrease of brilliancy would seemingly be the index to its alternating periods of activity and quiescence. In any case, it is probable the spot shines with intrinsic light, and it would be an important feature in connection with the progress of these inquiries to witness the transit of a satellite-shadow across it. The projection of such a shadow cone upon the spot might not obliterate it entirely, whence we should infer that its excessive lustre must be due to some other agency than reflection.

The lengthening period is quite opposed to any thesis which attributes to the spot a permanent situation on *Jupiter*. It must in some sense be atmospheric, for whatever it may be, and whatever conditions affect its existence, it cannot be an integral part of the planet, but is more probably a mass or volume of matter quite detached, though formerly evolved, from the surface. The fact that it moves with much greater velocity than the red spot leads us to the inference that, while the latter is on the extreme outer envelopes of the planet, the white spot is amid the lower strata, and rendered conspicuously visible by the convulsions which it is evident are of frequent occurrence on the equator. And one of the most remarkable features of this object is its long duration in a region where many changes are apparently operating to oppose its persistency. We have evidence of this in the different aspect which the spot assumes at short intervals, and in the oscillatory movement to which it is commonly subject.

There can be little question that this particular marking existed for some years before it came under general observation in the autumn of 1880, for the records and drawings of the few preceding oppositions contain obvious reference to an object of precisely similar character and position. Thus Prof. Hough's results at the Dearborn Observatory in 1879 include figures of *Jupiter* on Sept. 20, $9^h\ 45^m$, and Oct. 15, $8^h\ 7^m$, Chicago mean times. In the former of these delineations the white spot is well shown on the extreme west limb of the planet, and in the latter it is seen very slightly in advance of the middle of the red spot, so that a conjunction of the two markings must have occurred a few hours previously. Now, computing back on the period of $44^d\ 14^h\ 37^m{\cdot}5$ we shall find that the present white spot would be in conjunction with the red spot on 1879, Oct 15: hence the absolute identity of the object seen on that date by Prof. Hough with the one now visible appears to be beyond question.

It was not so regarded by him* however, on account of a marked increase of rotation period which must have taken place during the oppositions of 1879, 1880, and 1881, and moreover between the epoch of 1880 Oct. 1 and Oct. 28, the spot, if the same, must have suffered a great retardation, as it came to the central meridian fully 30 minutes after its computed time. This represents an increase of longitude of about 18° in 27 days, which is rather startling if we admit its actual occurrence. But in view of the "sudden deviations in its apparent place," which Prof. Hough and others have frequently observed, we may well consider it probable that the objects seen by him in 1879 and 1880 were identical with that now visible. In corroboration of this idea we have the conspicuous appearance of the spot detected by him in the earlier months of the series, and the fact that it is the only one depicted in his sketches. Besides, the position of his spot of 1879 indenting the great S. dark belt is exactly the same as that of the spot of 1880, which, from its brilliant appearance and obtrusive position with regard to this belt, could hardly fail to arrest the common attention of observers.

The rotation periods of the equatorial white spots observed by Prof. Hough were as follows :—

Interval.	Period of Rotation.
	h m s
1879, Sept. 10–1880, July 8	9 50 3·5
1880. July 8–1880, Oct. 1	9 50 0·5
1880, Oct. 28–1881, Jan. 30	9 50 9·8
1881, July 22–1882, Mar. 31	9 50 9·8

Prof. Hough would now, however, appear to regard several of these objects as identical, notwithstanding the marked extension of period, for he says ("Report of Dearborn Observatory, 1883," p. 12), "The equatorial white spot which was first observed in 1879 was again visible during the past opposition [1882–3]. The rotation period as deduced last year, viz. $9^h\ 50^m\ 9^s{\cdot}8$ satisfies the observations."

The increasing period of the white spot (which Professor Hough's observations since 1880 October 28 do not corroborate), representing as it does a considerable retrograde movement on the planet, is very interesting, as the same phenomenon is well defined in regard to the red spot, proving that whatever the origin may be, it operates in both cases, and apparently in nearly similar degree according to my own observations. The further

* He notes: "There are two principal spots, differing in longitude about 15° and in latitude about 1″." If, therefore, the spot observed by him for rotation in 1879 and 1880 is not absolutely the same as the one observed in later years, it must have had a much shorter period than at present, inasmuch as it preserved the same approximate difference in longitude from the similar spot which before 1880 Oct. 1 certainly indicated a period not greater than $9^h\ 50^m\ 3^s{\cdot}5$.

investigation of the longitudinal drift in these markings and its rate of increase will be an important object of future investigation.

Though I have occasionally observed other spots upon the planet, and recorded their times of transit, they have generally been too evanescent to be followed long. A very curious discordance of periods has been exhibited, but they are none the less certain, and it may be useful to present some of the results in a tabular form to show the differences referred to. The various periods are corrected for equation of light, differences of Jovicentric longitude, and phase :—

Ref.	Interval.	Number of Rotations.	Period of Rotation.
			h m s
a	1878, Sept. 25–1883, Dec. 5	4586	9 55 36·2
b	1880, Oct. 31–1881, Jan. 16	186	9 55 17·9
c	1882, Oct. 30–1882, Nov. 26	66	9 51 47
d	1882, Nov. 1–1882, Nov. 26	61	9 50 17
e	1880, Oct. 20–1883, Nov. 25	2760	9 50 8·7
f	1880, Oct. 29–1881, Jan. 7	171	9 48 0·0
	1880, Oct. 29–1881, Jan. 8	174	9 47 58·2

a. The great red spot in the S. hemisphere.
b. A short dark belt or elliptical spot s.p. the red spot.
c. A bright spot slightly N. of the equator.
d. A dark patch on the equator.
e. The equatorial white spot (slightly S. of the equator).
f. Two dark spots on a belt about 25° N. of the equator. These objects moved with greater velocity than any other markings. Relatively to the red spot they completed a revolution around *Jupiter* in 32^{d} 10^{h}. The rotation period of these spots seems to have been not only less than that of any other markings visible of late years, but also of any markings ever seen on the planet and observed for rotation.

From the above summary it is clear that the periods of different spots vary to the extent of 7^{m} 38^{s}, and it is fair to conclude that these limits admit of considerable increase by further observation. Whether the shorter periods are more nearly representative than the longer ones of the true rotation-period of the planet's sphere must remain a matter of opinion in the absence of more definite evidence on the subject. It seems to me very probable that the real length of the Jovian day is even shorter than that of any period which has been hitherto indicated by the various atmospheric markings. It is more easy to admit that such markings have a tendency to fall into arrear of the great velocity of the globe of *Jupiter* than that objects of such permanency as the white spot have a direct motion of more than 250 miles per hour upon the planet. The behaviour of

certain spots proves their disposition to lengthen out in longitudinal directions, and it is significant that this extension appears invariably to take place on the following side. The curious offshoot from the great S. dark belt which was seen in the autumn of 1880 N. of the f. end of the red spot developed into a very prominent belt by the constantly increasing prolongation of its E. end. And the dark patches near the equator generally run from the S. equatorial belt in an oblique direction to the E.; in fact all the visible markings seem to be more or less influenced by this tendency. The behaviour of these objects is evidently controlled by the rapid axial rotation of the planet from which they are evolved. And the true period of this rotation is at present unknown, but is probably much shorter than that usually adopted, though it may never admit of accurate determination, owing to the seemingly persistent obscuration of the planet's real surface.

Bristol: 1883, *Dec.* 24.

On Humidity as a Cause of Variation in the Rate of Chronometers.
By Major-General J. F. Tennant, R.E., F.R.S.

(*Extract from a letter to the Secretary.*)

I recently sent a paper on the change of rate of a chronometer which was due to the humidity of the air. Posssibly this may be in time to be added as a postscript if the paper be printed in the *Notices*.

The change to dry weather has been accompanied by the corresponding change of rate, that from October 22 to November 1 having been $-3^{s}\cdot41$ (losing). I have also heard from Mr. F. Chambers, at Bombay, who has examined the rates of sixty-three chronometers, and in five cases has found rates which are certainly independent of the temperatures, but which do appear to depend on the humidity. He finds (what my results show, though not conclusively) that the variation increases as time passes. If this be confirmed it is almost certain, I conceive, that the variation of rate is due to a gradual change in the oil.

Calcutta:
1883, *Nov.* 12.

Ephemeris for finding the Positions of the Satellites of Uranus, 1884. By A. Marth.

The angle of position P of the minor axes, the major and minor semi-axes a and b of the apparent ellipses described by the satellites, and the latitude of the Earth above the assumed plane of their orbits, are the following:—

Greenw. Noon. 1884.	P	*Ariel.* a_1	*Ariel.* b_1	*Umbriel.* a_2	*Umbriel.* b_2	*Titania.* a_3	*Titania.* b_3	*Oberon.* a_4	*Oberon.* b_4	Latit. of Earth.
	°	″	″	″	″	″	″	″	″	°
Jan. 9	285·59	14·76	+3·11	20·56	+4·33	33·72	+7·11	45·09	+9·51	+12·17
19	·59	14·88	3·11	20·74	4·34	34·01	7·11	45·48	9·51	12·07
29	·60	15·00	3·09	20·90	4·30	34·28	7·05	45·84	9·43	11·87
Feb. 8	·60	15·10	3·04	21·04	4·23	34·51	6·94	46·16	9·28	11·60
18	·60	15·19	2·97	21·16	4·13	34·70	6·78	46·41	9·07	11·27
28	·60	15·25	2·88	21·24	4·01	34·84	6·58	46·59	8·80	10·89
Mar. 9	·59	15·28	2·78	21·29	3·87	34·92	6·33	46·69	8·48	10·47
19	285·59	15·29	+2·66	21·30	+3·71	34·93	+6·09	46·72	+8·14	+10·04
29	·58	15·27	2·55	21·27	3·55	34·89	5·83	46·65	7·79	9·61
Apr. 8	·57	15·22	2·44	21·21	3·40	34·78	5·57	46·51	7·45	9·21
18	·57	15·15	2·33	21·11	3·25	34·62	5·33	46·30	7·13	8·85
28	·56	15·06	2·24	20·98	3·12	34·42	5·12	46·03	6·84	8·55
May 8	·55	14·96	2·16	20·83	3·01	34·17	4·94	45·70	6·61	8·32
18	·54	14·84	2·11	20·67	2·93	33·90	4·81	45·33	6·43	8·16
28	285·54	14·70	+2·07	20·49	+2·88	33·61	+4·73	44·94	+6·32	+8·09

The longitudes, $u-U$, of the satellites reckoned in their orbits from the points which are in superior conjunction with the planet's centre, are approximately the following:—

Greenw. Noon. 1884.	*Ariel.* u_1-U	Diff.	*Umbriel.* u_2-U	Diff.	*Titania.* u_3-U	Diff.	*Oberon.* u_4-U	Diff.
	°	°	°	°	°	°	°	°
Jan. 9	184·90	1428·46	31·86	868·74	26·71	413·53	221·03	267·39
19	173·36	·43	180·60	·73	80·24	·52	128·42	·38
29	161·79	·41	329·33	·71	133·76	·50	35·80	·37
Feb. 8	150·20	·38	118·04	·68	187·26	·48	303·17	·36
18	138·58	·36	266·72	·66	240·74	·47	210·53	·35
28	126·94	·33	55·38	·65	294·21	·46	117·88	·34
Mar. 9	115·27	·30	204·03	·64	347·67	·46	25·22	·34
19	103·57	·28	352·67	·62	41·13	·45	292·56	·33
29	91·85	·26	141·29	·60	94·58	·45	199·89	·33
Apr. 8	80·11	·24	289·89	·60	148·03	·44	107·22	·34
18	68·35	·23	78·49	·60	201·47	·45	14·56	·34
28	56·58	·23	227·09	·61	254·92	·46	281·90	·35
May 8	44·81	·22	15·70	·60	308·38	·47	189·25	·36
18	33·03	1428·22	164·30	868·61	1·85	413·47	96·61	267·37
28	21·25		312·91		55·32		3·98	

These values are to be interpolated for the times for which the positions of the satellites are required. The position-angles p and distances s are then to be found by means of the formulæ—

$$s \sin (p-\mathrm{P}) = a \sin (u-\mathrm{U})$$
$$s \cos (p-\mathrm{P}) = b \cos (u-\mathrm{U}).$$

The satellites move in the direction of *in*creasing position-angles, and will be at their greatest elongations ("N" in posit. P+90° and "S" in posit. P−90°) and at their superior and inferior conjunctions with the planet about the following Greenwich mean times :—

Ariel.

1884.	N.	S.		N.	S.		N.	S.
	h	h		h	h		h	h
Jan. 8	8·1	9 14·3	Feb. 25	5·3	26 11·6	Apr. 13	2·7	14 8·9
10	20·5	12 2·8	27	17·8	29 0·0	15	15·1	16 21·4
13	9·0	14 15·3	Mar. 1	6·3	2 12·5	18	3·6	19 9·9
15	21·5	17 3·8	3	18·8	5 1·0	20	16·1	21 22·4
18	10·0	19 16·2	7	7·3	7 13·5	23	4·6	24 10·9
20	22·5	22 4·7	8	19·8	10 2·0	25	17·1	26 23·4
23	11·0	24 17·2	11	8·2	12 14·5	28	5·6	29 11·9
25	23·5	27 5·7	13	20·7	15 3·0	30	18·1	
28	11·9	29 18·2	16	9·2	17 15·5	May		2 0·4
31	0·4		18	21·7	20 4·0	3	6·6	4 12·9
Feb.		1 6·7	21	10·2	22 16·5	5	19·1	7 1·3
2	12·9	3 19·2	23	22·7	25 4·9	8	7·6	9 13·8
5	1·4	6 7·6	26	11·2	27 17·4	10	20·1	12 2·3
7	13·9	8 20·1	28	23·7	30 5·9	13	8·6	14 14·3
10	2·4	11 8·6	31	12·2		15	21·1	17 3·3
12	14·9	13 21·1	Apr.		1 18·4	18	9·6	19 15·8
15	3·3	16 9·6	3	0·7	4 6·9	20	22·1	22 4·3
17	15·8	18 22·1	5	13·2	6 19·4	23	10·6	24 16·8
20	4·3	21 10·6	8	1·7	9 7·9	25	23·1	27 5·3
22	16·8	23 23·1	10	14·2	11 20·4	28	11·6	29 17·8

Umbriel.

	N.	S.		N.	S.		N.	S.
	h	h		h	h		h	h
Jan. 9	16·1	11 17·8	Feb.		1 11·1	Feb. 24	6·1	26 7·8
13	19·5	15 21·2	3	12·8	5 14·5	28	9·6	
17	23·0	20 0·7	7	16·3	9 18·0	Mar.		1 11·3
22	2·4	24 4·2	11	19·7	13 21·4	3	13·0	5 14·8
26	5·9	28 7·6	15	23·2	18 0·9	7	16·5	9 18·2
30	9·3		20	2·6	22 4·4	11	20·0	13 21·7

1884.	N. h	S. h		N. h	S. h		N. h	S. h
Mar. 15	23·4	18 1·2	Apr. 13	23·7	16 1·4	May 8	20·5	10 22·3
20	2·9	22 4·6	18	3·2	20 4·9	13	0·0	15 1·7
24	6·4	26 8·1	22	6·7	24 8·4	17	3·5	19 5·2
28	9·8	30 11·6	26	10·1	28 11·9	21	6·9	23 8·7
Apr. 1	13·3	3 15·0	30	13·6		25	10·4	27 12·1
5	16·8	7 18·5	May		2 15·3	29	13·9	31 15·6
9	20·4	11 22·0	4	17·1	6 18·8			

Titania.

Super. Conj.	h	N. Elong.	h	Infer. Conj.	h	S. Elong.	h
Jan. 8	8·5	Jan. 10	12·7	Jan. 12	17·0	Jan. 14	21·2
17	1·4	19	5·7	21	9·9	23	14·1
25	18·4	27	22·6	30	2 8	Feb. 1	7·1
Feb. 3	11·3	Feb. 5	15·6	Feb. 7	19·8	10	0·0
12	4·3	14	8·5	12	12·7	18	17·0
20	21·2	23	1·5	25	5·7	27	9·9
29	14·2	Mar. 2	18·4	Mar. 4	22·7	Mar. 7	2·9
Mar. 9	7·2	11	11·4	13	15·6	15	19·9
18	0·1	20	4·4	22	8·6	24	12·9
26	17·1	28	21·3	31	1·6	Apr. 2	5·8
Apr. 4	10·1	Apr. 6	14·3	Apr. 8	18·6	10	22·8
13	3·0	15	7·3	17	11·5	19	15·8
21	20·0	24	0·3	26	4·5	28	8·8
30	13·0	May 2	17·2	May 4	21·5	May 7	1·7
May 9	6·0	11	10 2	13	14·4	15	18·7
17	22·9	20	3·2	22	7·4	24	11·7
26	15·9	28	20·1	31	0·4	June 2	4·6

Oberon.

Super. Conj.	h	N. Elong.	h	Infer. Conl.	h	S. Elong.	h
Jan. 0	17·6	Jan. 4	2·4	Jan. 7	11·2	Jan. 10	20·0
14	4·7	17	13·5	20	22·3	24	7·1
27	15·9	31	0·7	Feb. 3	9·4	Feb. 6	18·2
Feb. 10	3·0	Feb. 13	11·8	16	20·6	20	5·4
23	14·2	26	23·0	Mar. 1	7·8	Mar. 4	16·6
Mar. 8	1·4	Mar. 11	10·2	14	19·0	18	3·7
21	12·5	24	21·3	28	6·1	31	14·9
Apr. 3	23·7	Apr. 7	8·5	Apr. 10	17·3	Apr. 14	2·1
17	10·9	20	19·7	24	4·5	27	13·3
30	22·1	May 4	6·9	May 7	15·7	May 11	0·5
May 14	9·3	17	18·1	21	2·9	24	11·6
27	20·4	31	5·2	June 3	14·0	June 6	22·8